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Risk Framing in Cardiovascular Medicine II

To the Editor: We read with interest the commentary by Alkhouli and Rihal.[1] The authors accurately remark about the need of easily understood tools, available to clinicians and patients at point of care, that could simplify the assessment of individual patient risk benefit and harm of an intervention. In particular, they observed the paucity of intervention-specific, individualized risk/benefit scores that facilitate the identification of higher risk individuals that benefit the most from an intervention, with acceptable probability of harm. Such an example is provided by the TIMI Risk Score for Secondary Prevention,[2] a risk score that identifies those patients who benefit from the addition of ezetimibe to statin therapy after acute coronary syndrome. Although those patients who have 2 or more risk indicators derive some benefit, those who have 0 or 1 do not. In contrast, it could be interpreted from their statement on the Table that the likelihood of no benefit from an intervention, which they calculated as 1 minus the absolute risk reduction, means that most (>97 %) patients are not likely to benefit on the trials exemplified. It has been observed that the absolute risk reduction (or any other measure of risk reduction) represents the average risk reduction in the study group, and given the logarithmic distribution of risk in a disease with overall outcome rate < 50%, it translates that approximately *one-third* of patients in a randomized clinical trial benefit from an intervention, the so-called Lake Wobegon effect.[3] It is important then to distinguish between the magnitude of risk reduction and the percentage of individuals in a population likely to benefit. We completely agree with the authors that we need the tools to identify with ease and clarity those patients who lie in that area of risk to communicate effectively with patients and share the decision whether to apply a given intervention.

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In reply—Risk Framing in Cardiovascular Medicine I and II

We thank the authors for their insightful comments on our perspective published in the journal.[1] We agree with Dr Modarressi[1] that sodium-glucose cotransporter-2 inhibitors indeed represent an important new treatment for patients with heart failure. Although we used the trial definition of major adverse cardiovascular events (cardiovascular death, nonfatal myocardial infarction, or stroke) in the text and in the table's footnote, we acknowledge that this was a secondary and not a primary end point.[1,2] The purpose of our viewpoint was to illustrate the issue of risk in absolute vs relative terms. We agree that a 4.3% absolute risk reduction of the composite end point of end-stage kidney disease, doubling of the creatinine level for ≥30 days, and death due to cardiovascular or renal disease is substantial from a population health view; however, it may be less substantial to the individual patient, especially considering the marked out-of-